As might be expected from the nature of the case, the section on Optics is the most complete. Mr. Wright's earlier book on "Light" is extremely instructive; the suggestions it contains are in many cases amplified and improved in the present work, and much that is new is added. There is one point, however, on which Mr. Wright does not express his meaning with his usual clearness. He says, when treating of the spectrum (p. 304), that it is in most experiments permissible to converge more light upon the slit by the use of a lens This statement is hardly sufficiently strong: it is not only a permissible course, but also the best course, to form on the slit an image of the source of light, using a lens of such a focal length that all the rays which pass through it, after converging to the slit, diverge so as all to pass through the lens and prism used to form the spectrum. In some cases it may be best to dispense with the condensers and lantern, and merely place the source of light close up to the slit itself. And again, in section 179, the rays emerging from the slit should not be a narrow parallel pencil, but a divergent pencil of such an angle as to fill the lens completely; while in Fig. 171 the light should not be shown as focussed on the prism, but on a screen behind the prism, and at about the same distance as the screen on which the spectrum is formed.

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## DRY DENUDATION.

*Die Denudation in der Wüste und ihre geologische Bedeutung: Untersuchungen über die Bildung der Sedimente in den Ägyptischen Wüsten.* Von Johannes Walther, a. o. Professor an der Universität, Jena. (Leipzig: S. Hirzel, 1891.)

THIS book forms the third part (being paged continuously) of the sixteenth volume of the Transactions of the Königlichen Sächsischen Gesellschaft der Wissenschaften. The questions propounded by the author for investigation are the following:—What meteorological forces are active in the deserts? What is their destructive effect on the rocks? What are the ultimate results of this? Is the present sculpturing of the deserts due to the influence of other forces than those which are now active? How can fossil deserts be recognized? The last is left unanswered; the others are considered in the light of information collected from books of travel, and from the author's own studies during two visits to the Egyptian desert.

In regard to rainfall, he points out that no part of the African desert is absolutely rainless, and that, as the storms, though rare, are heavy, the mechanical effects of water are more marked than they would be in a region where precipitation was more uniform. But in a desert, where the absence of plants and of soil exposes the rock to the effects of atmospheric variations, changes of temperature are yet more potent in causing denudation. These changes, owing to the dryness of the air, are great. The diurnal range may be full 30° C., the annual as much as 70° C. By the constant expansion and contraction due to these variations, the rocks are split, and the results are more important in producing denudation than are chemical changes. The author gives a number of illustrations to show how rock-masses in the desert are destroyed by the action of heat and cold, wind, and drifting sand. The surfaces of old walls are corroded; strata of different hardness in the face of a cliff are worn back unequally; masses of rock are isolated, and the blocks and pillars are carved into strange forms; denudation, in short, seems to proceed as actively in a desert as in a damp climate, and along very much the same lines. Isolated hills of tabular form are also very characteristic of desert denudation. These may be either on a large scale—outliers of an extensive plateau—or on a small one, like models, but a few feet high. In each case the cause is the same: a harder stratum at the top has preserved the softer material below. The author also describes the valleys of the desert, usually dry, and the cirques which, as was pointed out some years since by Mr. Jukes Browne (*Geol. Mag.*, Decade 2, vol. iv. p. 477), seem to occur in the deserts of Egypt even as in regions where ice may be supposed to have acted. The description of the latter is important, since it indicates that there is not that necessary connection between glaciers and cirques which some geologists appear to have imagined.

In regard to the excavation of the valleys, and perhaps of some other physical features on a larger scale, we think that the author is disposed to attribute rather too much to the sculpturing effect of heat, cold, and wind (with which of course drifting sand is included), and to overlook the probability that many of the bolder physical features may be memorials of an age when the region was well watered. It is often more than probable that deserts have not always been deserts, but that they are districts of local desiccation, which sometimes may be still in progress. So it is with the Aralo-Caspian area; the Great Salt Lake of Utah and the Dead Sea are mere remnants of fresh-water lakes on a far grander scale. When a large part of the present Jordan valley was one sheet of water, when glaciers replaced the cedars of Lebanon, and the peaks of Sinai were white with perennial snow, the Egyptian desert must have been no longer arid, and permanent streams must have occupied the wadis. Probably since that time the change in the physical features, though doubtless not unimportant, has been in the main superficial; the effects of the arid epoch through which Egypt and some other regions are passing may be compared to those of the Glacial epoch further north. The work has been that of the file rather than of the chisel.

That changes of temperature and the action of drifting sand have been agents of denudation of considerable importance was, of course, well known, and is stated in most text-books of geology, but Prof. Walther has done good service in emphasizing the fact, and in collecting together a number of interesting observations and illustrations which will be useful for reference, especially to teachers.

T. G. B.

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## OUR BOOK SHELF.

*The American Race.* By Daniel G. Brinton. (New York: N. D. C. Hodges, 1891.)

By "the American race," Dr. Brinton means the aboriginal race of America, and in the present work he makes what he believes to be the first attempt to classify it systematically on the basis of language. Whether lan-

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guage is, as he contends, "the only basis on which the subdivision of the race should proceed," is a question which needs rather fuller discussion than Dr. Brinton has devoted to it. It may, however, be admitted that in the study of the American aborigines, at the present stage of ethnographical science, no other test works so well upon the whole as the linguistic; and that the results attained by the use of it are interesting and instructive, whether they exactly correspond to racial distinctions, in the strict sense of the expression, or not. Dr. Brinton divides the American race into five great groups—the North Atlantic group, the North Pacific group, the Central group, the South Pacific group, and the South Atlantic group. Taking each of these in turn, he describes the various stocks included in them, paying attention especially to those portions of the continent about which ethnographers are least united. The facts are sometimes presented in a style unnecessarily dry; but the author has taken immense pains to arrange them clearly, and the work will be of genuine value to all who wish to know the substance of what has been found out about the indigenous Americans. We may note that Dr. Brinton does not agree with those who hold that the Red Men originated in America. On the other hand, he sees no reason to suppose that they came from Asia. His theory is, that they were an offshoot from the race which in the earliest times occupied Western Europe, and that they crossed from the one region to the other on a land-bridge. The former existence of this land-bridge he accepts chiefly on the evidence collected by various English geologists.

*Charts of the Constellations.* By Arthur Cottam, F.R.A.S. (London: Edward Stanford, 1891.)

THE large edition of Mr. Cottam's magnificent charts has met with a well-deserved success, and the smaller edition before us will doubtless be received in a similarly favourable manner. The thirty-six charts in the large edition measure 30 × 22 inches, and were projected on the scale of one-third of an inch to a degree of a great circle; the size in the reduced form is 15 × 12 inches, and the scale is one-half as great. In this handy size the charts will be sure to have a wide circulation among beginners in the study of astronomical science, and to possessors of the smallest telescope with alt-azimuth mounting they are invaluable. The road to an intimate knowledge of the stellar universe is considerably straightened by showing each constellation on a separate chart, whilst three key maps exhibit the relative positions of the various constellations, and will be extremely useful to show alignments over larger spaces than are included on the separate charts. Some notes on the constellations, in conjunction with an excellent introduction explanatory of various astronomical measurements and methods, enhance the value of the charts and help to render the work superior to any hitherto published. G.

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## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### Co-adaptation.

THE remarks on this subject made by Dr. Romanes (NATURE, March 26, p. 489) do not appear to me to present the case in a fair light, and I therefore think it advisable to make some further observations. In the first place, I do not allow that there is any "fortuity" in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual, for every variation, of whatever kind, must of necessity entail numerous structural and functional modifications. If variations in the length of neck or leg or wing, in the strength of certain muscles, or even in colour, arise "fortuitously" (meaning thereby through some unknown cause antecedent to birth), there must be corresponding variations in the length of the bones, nerves, blood-vessels, &c., as well as in the physiological processes upon which depends the supply of nutriment to the parts in question. If any variation survives, or is perpetuated by breeding, it must therefore possess not only the external visible character A, but also all the concomitant and less obvious or altogether undiscoverable modifications, B, C, D, &c. However complex the correlated modifications may be, the mere existence of the variation implies the existence of such modifications, and the survival of the variation implies the survival of these correlated modifications. If, therefore, it is argued that any variation is a simple case of modification in some one character only—as might be inferred from Dr. Romanes's letter—this is an unwarranted assumption entirely opposed to fact.

But the case we have to consider is that in which the variations A, B, C, D, &c., are not physically or physiologically correlated, but in which they are supposed to be "independent variables." It is to such cases that Mr. Spencer applied his objection. He says ("Factors of Organic Evolution," p. 14):—"What shall we say of co-operative parts which, besides being composed of different tissues, are remote from one another? Not only are we forbidden to assume that they vary together, but we are warranted in asserting that they can have no tendency to vary together. And what are the implications in cases where increase of a structure can be of no service unless there is concomitant increase in many distant structures, which have to join it in performing the action for which it is useful?" Then comes the case of the giraffe, in which Mr. Spencer endeavours to show that the fore- and hind-quarters of this animal must be regarded—if I may venture to express his views briefly—as, "independent variables."

In reply to arguments of this kind, it may be pointed out that mere remoteness does not preclude the possibility of there being correlated variability between structures or functions. And in the next place even if it be admitted that certain parts now co-operative were independently variable at first—as in many cases they no doubt were—it is a gratuitous assumption to suppose that they must have originated from the very beginning in individuals in which the parts were co-operative, *i.e.* in individuals in which the non-correlated variations, "A, B, C, D, &c.," occurred simultaneously. The chances against such variations occurring in any one individual are, I concede most fully, "infinity to one." But if A (with its correlated physical and physiological modifications) survives through natural selection (utility) or is bred by artificial selection, that form is fixed by virtue of its possessing certain characters. If it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio.* Thus with the giraffe it must be shown that the heightening of the fore-quarters not only conferred no particular advantage, but may even have placed the animal at a disadvantage unless associated with the necessary modifications of the hind-quarters, the necessary modifications being, according to Mr. Spencer, those required to enable the animal to escape pursuit, "since any failure in the adjustment of their respective strengths [the fore- and hind-quarters] would entail some defect in speed and consequent loss of life when chased." To me—and I speak with all deference to the views of Mr. Spencer—this seems like an imposition on Nature of certain conditions which might not be true. It cannot be dogmatically laid down that at the time of its development from the ancestral form the necessity for escaping pursuit was of equal or greater importance than the faculty of high-reaching. In fact, the heightening of the fore-quarters is just as likely to have *diminished* the necessity for great speed, by enabling the animal to take a wider survey of its surroundings, and so to get sight of its enemies in time to escape. But, if speed were essential for the preservation of the species, I must confess that I see no difficulty in the belief that this character also might be superadded subsequently to the heightened fore-quarters in the same way that other associations of characters arising from "independent variables" are fixed by artificial selection. I do not admit therefore that "all this is quite wide of the mark"; on the contrary, I believe that it is *the* mark, and that the particular case of co-adaptation quoted by Mr. Pascoe (I am afraid in a somewhat garbled form) may resolve itself into a "confluence of adaptations." In fact, I do not think that it

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